MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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The Rev. R. MAIN, President, in the Chair.

J. Stuart Stuart Glennie, Esq., 6 Stone Buildings, Lincoln's Inn;
Henry George Bohn, Esq., York Street, Covent Garden;
Philip E. Sewell, Esq., Fuente del Mar, Santander;
John M. Stothard, M.D., 11 Lower Sherrard Street, Dublin;
E. J. Stone, Esq., First Assistant, Royal Observatory, Greenwich; and
C. P. Mason, Esq., Denmark Hill Grammar School,

were balloted for and duly elected Fellows of the Society.

On the Binary Star η Cassiopeæ.
By Eyre B. Powell, Esq.

I believe up to this time no orbit has been put forward for the binary star, *η Cassiopeæ;* I am aware of attempts having been made in past years to arrive at elements by two or three individuals beside myself, but the results proved altogether unsatisfactory, owing to the irregularities existing among the early observations, especially those of distance. I now do myself the pleasure of laying before the Society an approximate orbit, at which I have arrived after the expenditure of no little trouble; but, in so doing, I wish it to be understood that, in consequence of the peculiar circumstances of the case, I do not claim for the elements any higher rank than that of a *first* approximation to the truth.

Some of the early distance measures are utterly irreconcilable with one another; and, perhaps, the most important, that of the founder of this branch of astronomy, Sir William

Herschel, is shown to be considerably too small by Kepler's law of areas. The angle of true anomaly, moreover, which has been described since 1779, falls short of 40°, and is so situated as not to afford a good grasp of the orbit. Any conclusions, therefore, which may be drawn at present must necessarily be insecure. Still, as it will take some thirty-five years for the *comes* to round the vertex of the apparent ellipse and reach its periastre, before which time a close approximation to the elements will scarcely be attainable, there seems no reason for deferring the attempt to reduce the motion in some degree to line and measure.

After some previous approximations I arrived at the following elements, using for the most part Sir John Herschel's graphical method, and calculating n and τ by means of different pairs of equations, the final sets of which afforded results in fair agreement with each other. Here I may remark in passing that this way of determining the mean motion and the time of periastral passage seems to me decidedly the best, when once the perspective ellipse has been fixed with tolerable accuracy.

Apparent Orbit.

Axis major	= 15″·8
Axis minor	= 9″·2
Maximum distance	= 12″·84
Position angle for do.	= 60°
Minimum distance	= 1″·3
Position angle for do.	= 288° 10′
C A, projection of semi-major axis of real orbit	= 6″·72
C B, projection of semi-minor axis of real orbit	= 6″·1
C S, projection of linear excentricity of real orbit	= 5″·18
Position angle for S A	= 252° 30′
Position angle for focal distance parallel to C B	= 190° 50′

Elements of Real Orbit.

$\varpi = 252° 30'$
$e = \cdot 770833$
$\Omega = 25° 33'$
$\gamma = 57° 59'$
$a = 10''\cdot 335$
$n = 1°\cdot 989$, P = 181 years nearly,
$\tau = 1715\cdot 0$

The annexed table exhibits the degree of correctness with which the elements represent the recorded motion of the star; in it I have taken the most reliable of the observations within my reach, and in many cases have used the means of nearly contemporaneous groups.

Date.	P_0	$P_c - P_0$	D_0	$D_c - D_0$	Observer.	Remarks.
1779·63	62° 4′	— 96′	11″·27	+ 1″·43	H	$P_0 = 60°\ 6'$ for 1782·4, according to h, in vol. v., R. Ast. Soc. *Memoirs*. The value here is from the *Phil. Trans.*
1803·1	70 46	+ 32	.....	.....	H	
1819·8	80 12	+ 10	10·8	+ ·13	Σ	
1825·78	83 5	+ 61	9·9	+ ·54	S	
1830·17	87 13	— 6	10·1	— ·04	h, Sm	h., 1829·43; Sm., 1830·91.
1833·3	89 1	+ 24	9·91	— ·14	Sm, D, Sm, Sm.	Sm., 1831·84; D., 1832·87; Sm., 1833 7; Sm., 1834·77.
1836·79	92 0	+ 6	9·43	+ ·01	Σ, Sm	Σ. and Sm. in admirable agreement. Σ., 1836·78; Sm 1836·81.
1843·19	95 48	+ 109	9·1	— ·28	Sm	On examining the interpolating position curve, P_0 is clearly seen to be too small.
1847·42	102 0	+ 47	8·65	— ·36	J	
1851·84	106 38	— 7	8·06	— ·07	J, J, J	1850·876; 1851·89; 1852·76.
1853·68	109 23	— 42	7·95	— ·21	J, J, P	J., 1853·14; P., 1853·92; J., 1853·99.
1856·0	112 26	— 56	7·6	— ·06	J, P	J., 1856·07; P., 1855·93.
1857·56	114 2	— 30	7·45	— ·11	J, J	1857·06; 1858·06.
1859·72	116 35	— 5	7·02	+ 1″·1	P	

H, Sir W. Herschel; Σ, Professor Struve; S, Sir J. South; h, Sir J. Herschel; Sm, Admiral Smyth; J, Captain Jacob; P, myself.

Owing to the unfavourable weather here, I have not secured any recent observations of η.

For the purpose of comparing the orbit with future observations I append the calculated places of the *comes* for the next five years.

Date.	Position.	Distance.
1861·0	118° 21′	6″·99
1862·0	119 51	6·89
1863·0	121 21	6·79
1864·0	122 58	6·69
1865·0	124 35	6·59

It is to be remarked, however, that the arc which will be described during the next ten or twelve years may be made, within moderate limits of error, to belong to different ellipses partaking of an osculating character.

Madras, December 1, 1860.

On the Three New Variable Stars, T *Bootis,* T *Serpentis, and* S *Delphini.* By Joseph Baxendell, Esq.

1. The first of these variables precedes *Arcturus* $1^m\ 45^s$, and is $11'\ 30''$ more south. It was mapped down as a $9\frac{3}{4}$ magnitude star on the 9th of April last, and on the 11th it was again seen and estimated to be of the 10th magnitude; but on the 22d it had diminished to the 12·8 magnitude, and on the following night it was invisible with Mr. Worthington's 13-inch reflector, in a sky which permitted stars of the 14th magnitude to be seen. It has frequently been looked for since, but not seen.

2. On the 12th of July last, while comparing with the heavens a chart of the cluster No. 72 of Sir W. Herschel's 8th class, I observed a star of the 11th magnitude in a position in which none was laid down on the chart, and on referring to the notes appended to my measures of the positions of the neighbouring stars I satisfied myself that this star was not visible, or, at all events, not brighter than the 13th magnitude in July and August 1859, when the chart was constructed. It had a deep yellowish red colour, which alone would have drawn attention to it had it been visible during my previous examinations of the cluster. For several days its brightness appeared to remain unchanged, but afterwards it diminished very gradually, and on the 13th of September it was no longer visible with Mr. Worthington's large reflector, and was therefore below the 14th magnitude. From the course of its curve of variation it appears probable that it had already passed a maximum when first seen on the 12th of July. Its place for 1860 is $18^h\ 21^m\ 59^s$, $+6°\ 12'·7$.

3. The third variable is No. 4351 Zone $+16°$ of the *Bonn Catalogue*, its place for 1855 being $20^h\ 36^m\ 24^s·1$, $+16°\ 34'·2$. On the 25th of September, 1860, I was surprised to find it a full magnitude brighter than I had ever before seen it, being equal to its neighbour, No. 4350, of the 8·3 magnitude, the two thus forming a fine but wide double star. From the 28th of September to the 8th of October it was thought to be slightly brighter than 4350, but in the beginning of November it had become sensibly less bright, and on the 18th of December it had diminished to the 11th magnitude, at which it has since remained. It was observed as a 9th magnitude star by Bessel on October 7, 1823, in Zone 192. In the *Bonn Catalogue* its magnitude is 9·5.

Manchester, Jan. 9, 1861.

Results of Meridional Observations of Small Planets; and Phenomena of Jupiter's Satellites; observed at the Royal Observatory, Greenwich, during the month of December, 1860.

(*Communicated by the Astronomer Royal.*)

Flora (8).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	o ′ ″
1860, Dec. 17	9 22 51·80	3 9 44·21	80 5 25·11
18	9 18 40·06	3 9 28·35	79 59 54·44

Psyche (16).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	o ′ ″
1860, Dec. 17	10 0 43·07	3 47 41·70	74 44 34·47
18	9 56 11·64	3 47 6·10	74 45 11·46

Massilia (20).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	o ′ ″
1860, Dec. 18	12 12 0·46	6 3 17·23	67 42 9·87
19	12 7 0·67	6 2 13·18	67 42 19·03
22	11 52 0·94	5 59 0·66	67 42 52·47

Euterpe (27).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	o ′ ″
1860, Dec. 5	7 34 20·41	0 33 36·30	88 29 48·58
6	7 30 54·23	0 34 6·12	88 25 15·89

Amphitrite (29).

Mean Solar Time of Observation.		Apparent R.A.	Apparent N.P.D.
	h m s	h m s	o ′ ″
1860, Dec. 8	9 31 44·57	2 43 9·42	65 1 52·08
18	8 48 22·98	2 39 6·30	65 33 46·62

No occultations of stars by the moon were observed during the month of December 1860.

Occultations and Transits of Jupiter's Satellites.

Day of Observation. 1860.	Satellite.	Phenomenon.	Mean Solar Time. h m s	Observer.
Dec. 18	I.	Tr. egress	12 2 23·86	E.
18	IV.	Occ. disapp. first cont.	12 18 21·26	E.
18	IV.	,, central bis.	12 21 20·76	E.
18	IV.	,, last cont.	12 23 20·44	E.
18	IV.	Occ. reapp. first cont.	17 9 28·47	A. D.
18	IV.	,, central bis.	17 12 3·03	A. D.
18	IV.	,, last cont.	17 14 17·67	A. D.

The initials E. and A. D. are those of Mr. Ellis and Mr. Davis.

On the Lunar Theory. By Sir J. W. Lubbock, Bart.* (Abstract.)

The memoir is chiefly occupied with the comparison and discussion of the numerical values of the coefficients of the several inequalities, as given by M. Plana and M. de Pontécoulant, and in the *American Tables*, Washington, 1853. It is shown what are the quantities which should be added to the longitudes and latitudes of the *American Tables* in order to obtain those which would be given by the coefficients of Pontécoulant, when quantities under $0''\cdot3$ are neglected; these corrections are so small that, practically, the places given by the *American Tables* may be considered as identical with the places which would be given by tables founded upon Pontécoulant's coefficients, and therefore by the theory of Lubbock and Pontécoulant. The author remarks:—"There is no error in M. Plana's analytical expression for the longitude in terms of the first, second, third, or fourth order. There are three errors of the fifth order, of which one is insensible. In M. Plana's analytical expression for the latitude there are no errors of the first, second, or third order, and only two of the fourth order. By carefully examining M. Plana's numerical reductions, M. de Pontécoulant and myself have succeeded in discovering the causes of the principal differences between M. Plana's coefficients and those of M. de Pontécoulant: there remain discrepancies in terms of the fifth and higher orders which it is very desirable should be got rid of. M. de Pontécoulant and myself, where we differ from M. Plana, went over our calculations so many times that we think the error can hardly be on our side; and unless M. Plana will revise

* The Memoir, which was read at the Meeting in November last, will probably appear *in extenso* in the *Memoirs* of the Society.—Ed.

his work, we must despair of this being ever accomplished: for no one again will ever possess the intimate knowledge which M. Plana has of the indirect method. As it appeared to me that astronomers would view with greater confidence a comparison of places given by the *American Tables* made by persons who could have no interest in enhancing their value, I made application to Mr. Hind, the Superintendent of the *Nautical Almanac;* and, in consequence, he directed Mr. Farley to procure places of the Moon from the *American Almanac*, and compare them with the observations made at Greenwich for the years 1856, 1857, and 1858; and as Mr. Hind has kindly allowed me to publish them with this paper, any one can see at once how extremely accurate the places given by these tables are, and how much more so than those given by Burckhardt's Tables. In these *American Tables* coefficients are employed, with very few exceptions, and those of no moment, founded on our labours — that is, M. Plana's, M. de Pontécoulant's, and my own — and due to theory alone. I am confident, therefore, that a joint posterity will give to us — that is, to Plana, Pontécoulant, and Lubbock, who, in 1846, furnished the means of constructing tables of the moon without any empirical hypotheses — the credit of first bringing the errors of the lunar theory within the errors of observation, and thereby bringing to perfection the solution of the problem of finding the longitude at sea by means of lunar observations."

The memoir contains an investigation, by means of the equations employed in the author's lunar theory (that is, in which the time is the independent variable), which sustains Prof. Adams' value of the second term $\left(=\frac{377}{64}\,m^4\,e'^2\right)$ in the expression for the acceleration. It contains also two examples of the determination of the inequalities of the reciprocal of the radius vector and the longitude.

The President gave a short account to the Meeting of the proceedings at the Radcliffe Observatory since the decease of the late M. J. Johnson, and his own appointment as Radcliffe Observer. Observations were made regularly with both the Meridian Instruments. The photographic registration of meteorological observations had been carried on without any interruption, and with great regularity. Several amendments which were required in the Heliometer had been successfully effected, and the instrument was then in good working order.

On Controlling Clocks by Electricity.
By Charles V. Walker, Esq.

Mr. Charles V. Walker gave a verbal address, which he has since, at the request of the Editor, kindly reduced into writing, as follows:—

"Mr. President,—You are fully aware—no one more so—that the clocks (certain of them, at least) on the South-Eastern Railway should exhibit very correct time. Over and above their ordinary duty as timekeepers, some of them have special service to perform in behalf of the Royal Observatory, with all of which you are personally very familiar, but which it may be interesting to explain to the Fellows of this Society, in so far as a certain clock in the Telegraph Office of the London Bridge Station is concerned, and which clock is under the immediate control of the prime clock in the Royal Observatory, on which error is not allowed to accumulate. I will first explain the special service required of this clock, and then describe the arrangements by which it is retained under the control of the distant clock in question, and made to show the same time, second by second.

"For the last ten years a very elaborate system of automatic distribution of time-signals, including the daily drop of a time-ball at Deal, has been in operation on the South-Eastern Railway. As far as I have been concerned, it originated in a conversation between myself and Mr. Glaisher on April 13, 1849, followed by a letter from him on May 10, in which he said, 'I also wish to talk with you about the laying down a wire from the Observatory to the Lewisham Station.' The question was taken up with great zeal and activity by the Astronomer Royal, and ere long the necessary agreements between the Admiralty and the Railway Company were executed. The Company very readily fell into our views, and erected four wires between Lewisham Station and London Bridge, charging for them merely the cost price; they charge the Observatory a few shillings annually for maintenance, and a few shillings for rent and right of way—the sums being merely nominal. The Observatory supply the Railway Company with a time-signal every hour, if required, and with other signals every second or two seconds, as necessities may dictate, for driving or controlling clocks. The Royal Observatory and Lewisham Station were connected by four gutta-percha wires, laid under Greenwich Park and across Blackheath. In course of time the buried wires became defective, and, to take their place, wires were suspended in the air, in 1860, from the Observatory to Greenwich Railway Station; and the railway wires were, at the same time, transferred from the North Kent Railway, which leads to Lewisham, to the Greenwich Railway. This route is now used. The first of the four wires is joined at the

London terminus to one of the Electric Telegraph Company's wires, which leads to Lothbury, whence signals are distributed throughout the kingdom; the second is used for controlling the clock that forms the subject of these remarks; the third is for the hourly time-signal on the South-Eastern district; and the fourth is spare.

"The wires along which the time-signals are distributed to the stations on the South-Eastern district are borrowed automatically by the aid of the clock at London, and for the ball-drop at Deal by the further aid of a clock at Ashford. They must be borrowed at the right time, and only for a short time; they cannot be spared long from their ordinary telegraph duties. And for the Deal drop it is further necessary that the borrowed wire shall for the time being be perfectly safe from the ingress of false signals from any of the telegraph stations, the clerks at which might, but for the precautions that have been taken, unwittingly send a speaking signal, and make a false drop at Deal, misleading the captains of ships then lying in the Downs.

"When the system first commenced, a Shepherd's electric clock was erected at the London terminus; it is without weight or spring, and its motive power was supplied, second by second, by the aid of the pendulum at the Royal Observatory, which in its oscillation made contacts with certain light springs, and so directed electric currents of the required character from batteries placed at Greenwich to electro-magnets placed within the clock at London. While the gutta-percha wire was new and in good condition, which was for a considerable time, the London clock would, for weeks, and often for months together, continue to show the same second as the Greenwich clock. We were able to verify this by the hourly time-signals on the other wire, already referred to, and which could be read off on a galvanometer placed inside the clock-case at London. This is a very remarkable illustration of the immunity from interruption that is enjoyed by ordinary telegraphic circuits. The failure of a single current would create an error of two seconds in our clock, and yet week after week true time was shown; so that $60 \times 60 \times 24 = 86400$ currents of electricity passed daily in safety from Greenwich Park to London Bridge. At last the gutta-percha began to succumb to time and other influences, and electricity was lost on the road. At first a solitary current now and then throughout the day failed; matters grew worse, the non-arrivals were more frequent, and our clock not only failed in its special purposes, but ceased to be an ordinary timekeeper, and we had often to cover the dial.

"I determined, in future arrangements, to abandon this bold experiment; for, all things considered, and notwithstanding its first successes, it was a bold experiment to place a distance of six miles between the motive power and the

machine—in this case not a small one—to be moved; and as we could not well abandon the clock, fitted as it was with all the apparatus for borrowing the telegraph wires at the selected hours for time-signals; and as we had no convenient place for it, if we had supplied it with the ordinary going power—a weight and a pendulum—I arranged that it should continue to be an electric clock, but should receive the necessary electric currents, second by second, by the aid of a pendulum oscillating in London instead of in the Royal Observatory—the clock to which this pendulum appertains being to be put under the control of the Greenwich clock. The clock selected for this work had originally been an electric clock. I furnished it with a wooden pendulum and a weight, and made other necessary alterations, furnishing it with a dead-beat 'scapement. I availed myself of the electric parts of the clock—at least of such portions of them as served my purpose—leaving them in their original places in the body of the clock, where they were so arranged as to act upon the crutch, and by its means on the pendulum. It will be understood from this that I did not select this mode of controlling a clock in any preference to the admirable method practised by Mr. Jones, of Chester (and adopted by Mr. Hartnup at Liverpool and Mr. Airy at Greenwich), who controls by a bar-magnet and a coil of wire near the lower end of the pendulum; but having to deal with a clock already possessed of electric parts, I sought out how I might best turn them to account *in situ*. This clock has acquired the name of the 'Mechanico-Electric Clock,' which serves very well as a distinctive name. It contains a U-shaped bar of iron, with its ends upward, and surrounded by coils of No. 16 copper wire; this wire is $\frac{1}{16}$ in. in diameter. A brass bar is fixed by its centre upon the arbor that carries the crutch; it is at right-angles to the arbor, and in a horizontal position. Any force applied to this bar moves the arbor; any force applied to the arbor moves the bar. At one end of the brass bar is a bar of soft iron, at right-angles to the brass bar, and also in a horizontal position; it in fact forms, with the brass bar, a T-piece. A counterpoise, capable of adjustment, is fitted to the other arm of the brass bar; the iron bar is situated above and facing the poles of the electro-magnet already described, and is attracted downwards whenever a current of electricity is present in the coils of wire, and, of course, reacts on the pendulum itself by means of the crutch, with which it is solidly connected. The pendulum is tolerably good, and measures very fair time for ordinary purposes. When the clock is set going and left to its own resources, it may vary a few seconds more or less either way during the day. All this time the iron bar or armature approaches to or recedes from the electro-magnet at each alternate oscillation of the pendulum, but is uninfluenced. If, now, from some foreign source, a current of electricity is made to circulate through

the coils at each alternate second, the iron bar, and with it the crutch and the pendulum, are perfectly controlled by these synchronous currents, and the 'mechanico-electric clock' is retained in strict companionship with the clock from which the currents are sent. In the preliminary experiments in the shop, a gaining or a losing rate of twenty or more seconds per hour was put upon the clock; but in either case it was perfectly controlled, and constrained to show the same time as that exhibited by the controlling clock.

"The manner in which the currents are put into circulation by the Observatory clock is simple enough. At London, a wire is led from the earth to one end of a zinc-graphite battery of a few cells; from the other side of the battery a wire goes to the commencement of the coil of the electro-magnet in the clock; from the end of the coil, a wire passes to one side of a galvanometer, fixed on the outer side of the clock-case. I may here mention that this galvanometer forms no necessary part of the system; it is placed here so that the motions of its needle at every alternate second may indicate to those in charge, that the circuits are in order and the currents are duly passing. They can detect also at a glance, by the feeble motion of the needle, when the batteries are getting low. From the other side of the galvanometer, the wire proceeds direct to the Royal Observatory, where it terminates in a slight spring, which is touched by the pendulum at every alternate second. The pendulum, by means of the frame of the clock, to which it is suspended, is connected by a wire to the earth. So that each time the pendulum touches the spring, the telegraph circuit is completed, and the battery current flows. This arrangement has been for several months in operation; and accomplishes all that we had proposed. Week after week, our clock has shown the same second as the clock at Greenwich. The control is effected at a very small expenditure of electric force.

"I have had some experience in the relations that have been established between electricity and time. The conclusions to which I have arrived are, that electricity is out of place as a moving power for clocks. It is an unnecessary refinement; and is far more troublesome than a weight or a spring. But electricity is of very great value as a controlling power. It is an unfortunate circumstance that the pendulums of 8-day dials, of which there are thousands in this country, oscillate in no fraction of a second. There is an established train of wheels; and stereotyped numbers for the wheels and pinions, and these are often intermixed. These dials are the staple time-keepers throughout the country, in offices and places of business. Dozens of them are often found under the same roof. With very little addition to the movement every such group could have been kept in perfect control by the one pendulum of a good regulator, if they were other than they are.

"There is one point in the control of clocks that must not

be overlooked. The same currents that will keep a pendulum that is once in beat always in beat, will also stop the same pendulum altogether, if they arrive at the wrong time; that is, if they arrive so as to oppose its movement. It will not do to establish the electric communication hap-hazard, when the pendulum is at any part of its arc; it must be made to commence so that the motion thus applied to the crutch may coincide or conspire with the then motion of the pendulum. Before erecting the clock, and while it was in the shop, I made many experiments on this point, and invariably stopped the clock, when I applied the current at the wrong time. I advised the Astronomer Royal of this peculiarity, and he made some trials on a clock in the Observatory, which was controlled near the bob, on Mr. Jones's plan; and with the same results. This delayed me for some months in putting my clock to the test of real work; because the wires between London and Greenwich were at that time still in a defective state; and I could not depend upon the regular appearance of currents. I made the trial; but when the currents flagged for a time and then recovered their strength, it happened as often as not that they reappeared at the wrong moment, and the clock was repeatedly stopped; so that I was glad to leave the clock to its own resources until the wires were repaired. Since that time, we have had no recurrence of the interruption save in one instance; when from some cause the Observatory clock was at rest for a short time, and of course ceased to give signals; and when again set in motion, the currents caught our pendulum at the wrong time.

"I have since made a provision against these contingencies; but have not had an opportunity to adjust it, nor, indeed, any occasion to bring it into operation. I have divided the Greenwich wire as it enters the clock, and before it arrives at the galvanometer. A pair of light springs are at the division. When the Greenwich pendulum touches the spring there, this pair of springs must also be in contact, or no current can pass. The springs are pressed together by the pendulum as it oscillates against them; and I have so arranged them that they cannot be pressed together by the pendulum unless it is in that particular position in its arc, when the controlling power is valuable. In no other position therefore, in no unfavourable position of the pendulum can the current enter, and the clock will not be stopped. All that happens is that the error, be it a gain or loss, accumulates till it amounts to two seconds; and then the Greenwich and London pendulums are again in beat together, and continue so."

Abstract of his latest Results. By Prof. Wolf.

(*Translation communicated by Mr. Carrington.*)

Some fine series of observations of Flaugergues, Adams, Arago, and others, have enabled me to fill in previous breaks, and to express in the same unit my Relative numbers (for the abundance of Solar Spots in successive years) for the years from 1749 to 1860. They are as follows:—

1749	63·8	1777	63·0	1805	50·0?	1833	7·5 *m*
1750	68·2 M	78	94·8	06	30·0?	34	11·4
51	40·9	1779	99·2 M	07	10·0?	35	45·5
52	33·2	1780	72·6	08	2·2	36	96·7
53	23·1	81	67·7	1809	0·8	37	111·0 M
54	13·8	82	33·2	1810	0·0 *m*	38	82·6
55	6·0 *m*	83	22·5	11	0·9	1839	68·5
56	8·8	84	4·4 *m*	12	5·4	1840	51·8
57	30·4	85	18·3	13	13·7	41	29·7
58	38·3	86	60·8	14	20·0?	42	19·5
1759	48·6	87	92·8 M	15	35·0?	43	8·6 *m*
1760	48·9	88	90·6	16	45·5 M	44	13·0
61	75·0 M	1789	85·4	17	43·5	45	33·0
62	50·6	1790	75·2	18	34·1	46	47·0
63	37·4	91	46·1	1819	22·5	47	79·4
64	34·5	92	52·7?	1820	8·9	48	100·4 M
65	23·0	93	20·7?	21	4·3	1849	95·6
66	17·5 *m*	94	23·9	22	2·9	1850	64·5
67	33·6	95	16·5	23	1·3 *m*	51	61·9
68	52·2	96	9·4	24	6·7	52	52·2
1769	85·7 M	97	5·6	25	17·4	53	37·7
1770	79·4	98	2·8 *m*	26	29·4	54	19·2
71	73·2	1799	5·9	27	39·9	55	6·9
72	49·2	1800	10·1	28	52·5	56	4·2 *m*
73	39·8	01	30·9?	1829	53·5 M	57	21·6
74	47·6?	02	38·3?	1830	59·1	58	50·9
75	27·5 *m*	03	50·0?	31	38·8	59	96·4
1776	35·2	1804	70·0? M	1832	22·5	1860	98·6 M

If these numbers are laid down graphically, there results a curve-line consisting of numerous waves. It is unquestionable that the length of these waves is variable, and all the epochs of minimum since the discovery of solar spots are very nearly given by the formula

$$\varepsilon_x = 1732{\cdot}823 + 11{\cdot}119 \,.\, x$$
$$+ 1{\cdot}621 \,.\, \sin\left(146^\circ + x \,.\, \frac{360^\circ}{15}\right)$$
$$+ 1{\cdot}405 \,.\, \sin\left(230^\circ + x \,.\, \frac{360^\circ}{5}\right)$$

If the two last variable terms in this formula are neglected, we have the mean epochs of Minimum; and if we compare these with the epochs given by the observations, and with the height of the Maxima expressed by the Relative numbers, we find there is a very remarkable connexion, which the following table places clearly in evidence: —

True Minimum.	True — Mean Minimum.	Diff.	Rel. No. of Max.	Diff.	True Maximum.
1744·5	+0·588				
		+	68·2		
1755·7	+0·639			+	1750·0
		—	75·0		
1766·5	+0·320			+	1761·5
		—	79·4		
1775·8	−1·449			+	1770·0
		—	99·2		
1784·8	−3·618			—	1779·5
		+	90·6		
1798·5	−1·037			—	1788·5
		+	70·0		
1810·5	−0·156			—	1804·0
		+	45·5		
1823·2	+1·425			+	1816·8
		—	53·5		
1833·8	+0·906			+	1829·5
		—	111·0		
1844·0	−0·013			—	1837·2
		+	110·4		
1856·2	+1·068			—	1848·6
			98·6		1860·5

The constant opposition of signs can hardly be regarded by any one as accidental, and if admitted to be regular, we have manifestly this law, — *greater activity on the Sun goes with shorter periods, and less with longer periods.* I believe this law to be one of the most important relations among the Solar actions yet discovered, and which, taken in connexion with the results for distribution in heliographical latitude, and corresponding apparent periods of rotation, promises to throw much light on this difficult subject, and perhaps even on the phenomena of Variable Stars.

Zurich, 1861, *Jan.* 19.

Mr. Carrington wishes to call attention to the question of the determination of the figure of the Sun, and suggests the measurement for this purpose of photographic pictures of the disk. By aid of a reference given by Mr. Carrington, the Editor has found as follows: —

The question is discussed in a memoir by Mossotti, "Formole per determinaire gli Assi del Sole supposto uno spheroide ellittico, con applicazioni," in the Appendix to the Milan Ephemeris for 1820. The figure is in the first instance assumed to be an ellipsoid with three unequal axes, but supposing it to be a spheroid of revolution the formulæ are as follows: (for greater convenience D_v, D_h, are written in the place of the single quantity D′, which is used in the memoir to denote successively the vertical and horizontal semi-diameters):

D denotes the equatoreal diameter as it would be seen at the mean distance 1; D_v and D_h the vertical and horizontal diameters reduced to the mean distance 1; e the excentricity of the spheroid ($= 1 - \frac{c^2}{a^2}$ if a, c, are the equatoreal and polar semi-diameters). Then

$$D - D_v = \tfrac{1}{2} e^2 D p^2, \quad D - D_h = \tfrac{1}{2} e^2 D p^2_2,$$

and consequently

$$\frac{D_v}{D_h} = \frac{1 - \frac{1}{2} e^2 p^2}{1 - \frac{1}{2} e^2 p_2},$$

or

$$D_h - D_v = \tfrac{1}{2} e^2 (p^2 D_h - p^2_2 D_v),$$

in which last formulæ, it would clearly be unnecessary to reduce the observed diameters to the mean distance 1, since the formulæ contain only the ratio of these diameters. The values of p^2, p^2_2, are calculated by the formulæ in the memoir with the data

Inclination of the solar to the terrestrial equator $= 25^\circ\ 40'$,
Right ascension of ascending node, ditto ditto $= 16\ 50$,

for the middle of each month; viz., the values are

	p^2	p^2_2
January	0·99	0·00
February	0·91	0·07
March	0·82	0·16
April	0·81	0·18
May	0·88	0·12
June	0·97	0·03
July	0·99	0·00
August	0·92	0·06
September	0·83	0·16
October	0·81	0·19
November	0·88	0·12
December	0·97	0·03

The observations of Littrow and Maskelyne referred to in the memoir make the figure a prolate spheroid; those of Carlini, an oblate spheroid.

In a paper in the *Ast. Nach.* t. ix. p. 365 (1831), M. Bianchi has applied the above formulæ to observations taken at Modena in the years 1828 and 1829: the figure is found to be an oblate spheroid; but the vertical diameters alone, the horizontal diameters alone, and the two conjointly, give for $\frac{1}{2}e^2$ the very discordant values $\frac{1}{1925}$, $\frac{1}{110}$, $\frac{1}{249}$. As to the deduction of the true ellipticity from the observed figure of the disk, see also Mr. Grant's "Note on the correction to be applied to the apparent ellipticity of a Planet, in consequence of the elevation of the Earth above the plane of the Planet's Equator," *Monthly Notices*, vol. xiii. (1853), pp. 192–196.

RECENT PUBLICATIONS.

Théorie du Mouvement de la Lune. Par M. Delaunay (premier volume) being t. xxviii. of the *Mém. de l'Acad. de Paris.*

The following is in substance the account of the work as given by the author in the *Comptes Rendus* (séance du 24 Déc. 1860):—

"I have already had occasion to explain the object of the great work which I undertook fourteen years ago on the theory of the Moon. It is known that my end was to effect a new analytical determination of the lunar inequalities carrying the approximations notably further than had been done before me. Concentrating at first all my efforts on the inequalities due to the disturbing force of the Sun, and admitting in the calculation that the motion of the Sun takes place according to the laws of elliptic motion, I proposed to myself to investigate all the parts of these inequalities which are not of an order superior to the seventh. M. Plana, in his great work on the Lunar Theory, had stopped at the inequalities of the fifth order. The method employed by me consists in a series of operations similar to each other, whereof each has for its object to get rid of one of the periodic terms of the disturbing function by the aid of a simple change of variables. When, by the application of this method, we have removed from the disturbing function the terms of most importance in regard to the magnitude of the inequalities to which they give rise, the question is simplified, and can then be treated as easily as if it related to the inequalities of a Planet or the Sun.

"The volume which is just printed contains a part of the formulæ which I have obtained by operating in conformity with the above method. It is composed of five Chapters. The first is devoted to the establishment of the differential equations, the integration whereof gives the inequalities of the motion of

the Moon; the second, to the development of the disturbing function and of the elliptic values of the Moon's co-ordinates; and the third, to the exposition of the analytical method which I have imagined for integrating the differential equations by dividing the work so as to enable the further development of the approximations in each part. These three Chapters, much less extensive than the others, furnish a kind of introduction to the actual working out of the method, which is developed in the following chapters. The fourth Chapter contains the complete development of the disturbing function, with the different modifications it has successively undergone by means of a succession of 57 operations effected upon it to disembarrass it of the most important terms; this development contains 460 terms. Lastly, the fifth Chapter contains all the details of the establishment of the formulæ relative to the 57 operations in question. The second volume, the printing of which I hope to commence forthwith, will contain, 1°, the different formulæ intended for taking account of the terms which remain in the disturbing function after the foregoing 57 operations have been effected; 2°, the expressions of the three co-ordinates of the Moon with *all* their inequalities to the seventh order inclusive for the longitude and the latitude, and to the fifth order for the reciprocal of the radius vector; 3°, lastly, several chapters intended to complete the expressions for the co-ordinates of the Moon by taking account of all that was provisionally neglected in order to the consideration of only the capital part of the question. Desiring that my work should inspire to astronomers as much confidence as possible, I have neglected none of the means which appeared to me proper for facilitating the verifications which may occur to any of them. In particular I may refer to the indications accompanying the different parts of the disturbing function (Chap. IV.), and which have for their object to show the origin of each part.

"The simplicity and regularity of the method which I have followed, for the inequalities of the Moon's motion, have permitted me to set forth the series of the operations with all desirable clearness, without the necessity of long explanations. Thus, the last two chapters of my first volume, which occupy seven-eighths of the entire volume, consist almost exclusively of formulæ, with very little text. Many of the formulæ are very long; in particular that which forms the whole of Chapter IV., and which covers 138 pages. The printing of the volume presented the greatest difficulties, and it has required all the skill and zeal of M. Bailleul, the Director of the Mathematical Printing Establishment of M. Mallet-Bachelier, to bring the work to a conclusion, and to render all the parts of the volume agreeable to the eye without injury to the perfect clearness of the formulæ."

The Radcliffe Catalogue of 6317 *Stars, chiefly Circumpolar, reduced to the epoch* 1845·0, *formed from the Observations made at the Radcliffe Observatory under the Superintendence of Manuel John Johnson, M.A., late Radcliffe Observer. With Introduction by the Rev. Robert Main, M.A., Radcliffe Observer.* Published by order of the Radcliffe Trustees. 8vo. Oxford, 1860.

The following are extracts from Mr. Main's Introduction:

"The Radcliffe Catalogue of stars contains the result of *all* the Star-observing at the Radcliffe Observatory from 1840 to the end of the year 1853. For the year 1854 only Fundamental Stars are included, and those which were necessary to make more complete the re-observation of Groombridge's Catalogue of Circumpolar Stars; and for the following years as far as 1859, in general such observations only were used as were necessary in order to complete the results of former years for the Stars already included in the Catalogue.

"The compilation of the Catalogue was commenced as early as the year 1850; and at the time of the death of Mr. Johnson the whole of the work was in the hands of the printer, and a few sheets were actually printed off under his superintendence. After his decease the printing of the remainder of the Catalogue went on without serious interruption until its completion, under the superintendence of Mr. Quirling, the First Assistant of the Observatory.

"The chief labour of the comparison of the proofs with the manuscript was performed by Mr. Luff, of whose most valuable and almost gratuitous labours mention is so frequently made by Mr. Johnson in the introductions to separate volumes of the Radcliffe Observations.

"The MSS. used in the compilation of the Catalogue have been with few exceptions bound up and labelled, and they admit of very easy reference.

"On the whole, . . . the equinox employed is that of the *Nautical Almanac* for 1840, without any attempt at independent determination by observations of the Sun.

"The result [of a comparison given in the introduction] is so far satisfactory as shòwing that the equinox to which the Right Ascensions of the Radcliffe Catalogue have been referred does not differ materially from that of the Greenwich Twelve-year Catalogue; but there is a variation in the differences of Right Ascension for different hours, which is too well marked to be accidental, and of which I am unable to give any account. From x^h to xv^h, namely, the mean of the difference is $0^s{\cdot}09$, while the mean for the remaining hours is only $0^s{\cdot}025$.

"There is no doubt but that this anomaly depends on something which has affected the Radcliffe observations or their process of reduction, because similar differences are found to

exist in the mean Right Ascensions of the Fundamental Stars as deduced by *Wolfers* for the year 1860, as compared with Johnson's results from the observations of 1854 and 1855 reduced to 1860, as given in the sixteenth volume of the Radcliffe Observations. Still the origin of it is very obscure, and after thinking of many possible causes I do not find one which is altogether adequate. It may be, however, remarked that the hours of Right Ascension for which the differences are greatest, are those which correspond to the Spring and early Summer months for evening observations of the stars in those hours.

"As the principal object which Mr. Johnson had in view in star-observing was the complete re-observation of the stars in Groombridge's Catalogue, all in that Catalogue have been looked for with great care, and with very few exceptions all have been re-observed.

"In consequence of the lamented death of Mr. Johnson, the publication of the Radcliffe Catalogue has been delayed considerably beyond the time which would have been necessary if he had been spared to see the printing of the work completed. In fact, the printing was completed about the middle of last year [1859], and has waited only for the introduction and general revision, which could be performed only by a person authorised to assume the direction of the Observatory. On my appointment to the office of Director in June last, the study of the details of the computations was one of my earliest cares; and I did not rest satisfied till I had convinced myself, by a searching scrutiny, of the accuracy of all the elements and of all the processes employed, and had put myself into a position enabling me to hold myself responsible for the general accuracy of the work. I found, however, that it would not be prudent to proceed to the publication of it until I had come into residence; and since that time, or from October 1st [1860], I have been employed upon it as incessantly as my other duties permitted; and I now confidently offer it to the world as a work well sustaining the reputation of the late Director, and as one of the most important and accurate of modern Star Catalogues."

Catalogo di 1321 *Stelle Doppie misurate col grande Equatoriale de Merz, osservate all' Osservatorio del Collegio Romano, e comparate colle Misure anteriori.* Del P. A. Secchi. Roma, 1860.

An account of the work is given by the author in the *Comptes Rendus*, t. li., (17 Dec. 1860). The work is the fruit of the observations of five years. It contains the measures of 1321 Double and Multiple Stars, made with the great equatoreal of Merz, and compared with the anterior measures of

Struve, Mädler, and other astronomers for the determination of the motions.

The following are some of the conclusions from this review of nearly 7600 complete observations of stellar systems:

1°. The number of Stars in the first four orders of Struve, the motion of which is established, is to the total number of Stars observed in the following proportion:

Order 1,	stars in motion,	to all the observed stars,	as 1 : 2
,, 2	,,	,,	1 : 3
,, 3	,,	,,	1 : 6
,, 4	,,	,,	1 : 12

The review of these four orders comprises all the *Lucidæ* and a great part of the *Reliquæ* of the *Mensuræ* of Struve; it embraces also a large number of the stars of the catalogues of Pulkova, of Herschel at the Cape of Good Hope, of Smyth, &c.

2°. As it is important to fix the attention of astronomers on the stars the motion of which is ascertained, in order to the perfecting of the observations, and the avoidance of a loss of time in the measurements of fixed objects, there is given at the end of the Catalogue a recapitulation of the Stars measured, distinguishing them into the classes of certain motion, of doubtful motion, or of no motion. Denoting them by Struve's numbers, the Stars of certain motion are,

First order.

L. Nos. 2 13 73 205 216 257 333 412 460 511 1356 1457
1670 1728 1819 1937 1938 1967 2055 2084 2215 1315 2438 2509
2574 2729 3062.
R. 234 236 278 840 1426 1457 1663 2402.

Second order.

L. Nos. 113 138 186 228 262 305 314 400 408 535 566 577
945 948 1037 1126 1157 1187 1196 1338 1348 1476 1517 1523
1555 1647 1687 1768 1781 1865 1883 1932 1944 1998 2032 2107
2114 2171 2281 2289 2369 2437 2525 2579 2744 2799 2881.
R. 183 208 498 1081 1757 1837 1876 2106 2356 2434 2491 2544
2662 2856 2934 3047.

Third order.

R. Nos. 91 202 389 572 742 997 1273 1424 1536 1777 1785 1788 1909 1954 1988 2021 2052 2130 2382 2383 2576 2603 2624 2644 2804 2909 3001 3050.

158 195 249 355 403 932 1439 1450 1658 1722 1842 2026 2097 2120 2205 2303 2309 2484 2541 2828 2900 2942 3046.

Fourth order.

L. Nos. 422 589 982 1066 1110 1263 1306 1543 1888 2272 2725 2822 2928 2944 3008.

R. 44 122 295 1300 1830 1925 2165 2455 2538 2877 2976.

Fifth order.

60 550 668 1516 2732 2708. —— 2220 2262 142.

These numbers include all the Stars the orbital motion whereof is known. But in order to decide as to many others, in particular of the *doubtful* ones, it will be necessary to wait another quarter of a century, the interval which separates these from the observations of M. Struve. The author proposes to continue his observations on the doubtful class, and for the more distant orders, the review whereof is still imperfect.

Mémoire sur la Détermination des Distances polaires des Etoiles Fondamentales. Par E. Laugier, Membre de l'Institut, Astronome adjoint du Bureau des Longitudes, &c. (forms part of t. xxvii. of the *Mémoires de l'Acad. des Sciences*), Paris, 1859.

The observations were made at the Paris Observatory in the years 1851–1853, with the Mural Circle of Gambey. The memoir is divided into two parts; the first contains, 1°, the description of the Mural Circle; 2°, the series of rough observations made by the author with the instrument; they are collected in tables placed at the end of the text; 3°, all the necessary details in regard to the instrumental corrections to be applied to the observations and in regard to the elements of reduction; 4°, the determination of the colatitude of the Mural Circle and of the polar distances of the observed Stars.

In the second part the author discusses the declinations of 140 stars, published in the different Catalogues, and he deduces a normal Catalogue with which he compares the others. Lastly, starting from the normal declinations he determines anew the colatitude of the Mural Circle by means of the zenith distances given in the first section.

The epoch of the normal polar distances is January 1, 1852. The colatitude, as deduced from the Circumpolar Stars, is 41° 9′ 48″·66 ± 0″·05, and as deduced from the normal positions and zenith distances of the Fundamental Stars is 41° 9′ 48″·72 ± 0″·04.

Posizioni Medie de 2706 *Stelle pel* 1° *Gennajo* 1860 *distribuite nella Zona Compresa fra* 10° *et* 12° 20′ *de Declinazione Australe dedotte delle Osservazione fatte negli Anni* 1856–57–58, *nell' I. R. Osservatorio di Padova.* Memoria di Giovanni Santini. 4°, Venezia, 1858. (Estr. dal volume vii. delle Memorie dell' I. R. Istituto Veneto.)

The memoir is a sequel to two others published in vols. v. and vi. of the *Nuovi Saggi dell' Accademia di Padova*, containing a catalogue of stars between the parallels of 10° north declination and 10° south declination. The epoch of the catalogue is, as stated, in the title, Jan. 1, 1860. The right ascensions and declinations of the observed stars are compared with those in the catalogue of Weiss.

Sur les Lois du Mouvement propre des Etoiles du Catalogue de Bradley. Par M. Kowalski. 8vo. pp. 1–90, 1859.

The object of the memoir is to investigate whether the proper motions, and consequently the true motions in space, of the Stars of Bradley's Catalogue, are subjected to any other laws than those which follow from the motion of the planetary system, and what consequences can be deduced from them with reference to the structure of the Stellar Universe and the forces which govern it. The results deduced by M. Mädler from the proper motions of the stars in the Catalogue in question serve as the basis of the investigations of the memoir. The Catalogue of Bradley contains 3222 stars; of these there are only 86 the proper motions of which are not known, and the materials are thus furnished by 3136 stars. The conclusions arrived at are not stated so explicitly, as that, without risk of misrepresentation, a *résumé* of them can be given, but the memoir will probably be read with interest by those who have occupied themselves with the speculations of Stellar Astronomy.

Note relative à une Courbe du Sixième Degré qui se présente en Astronomie. Par E. de Jonquières. Tortolini, t. i. pp. 112–116. (1858.)

The formula for the Equation of Time, as a function of the true longitude of the Sun, was put by Lagrange in the form —

$$E = -462 \sin(\varphi - \alpha) - 593 \sin 2\varphi - 3 \sin 2(\varphi - \alpha) + 13 \sin 4\varphi,$$

where φ is the longitude of the sun, α that of the aphelion [? apogee], and E is given in seconds of time.

Neglecting the last two terms, which are of comparatively small magnitude, and putting $\frac{1}{2}\frac{E}{593} = \varrho$, $\frac{231}{593}\sin\alpha = a$, $\frac{231}{593}\cos\alpha = b$, this gives the equation

$$\varrho = a\cos\varphi - b\sin\varphi - \sin\varphi\cos\varphi,$$

which may be considered as the polar equation of a curve, the radius vector whereof is proportional to the equation of time.

The note belongs, not to Astronomy, but to Modern Geometry, and contains a very interesting discussion, from that point of view, of the properties of the above-mentioned curve; but the figure of the curve, in which the radius vector gives the value of the equation of time on the scale of one minute of time to a centimetre of length, shows very distinctly the march of the equation of time throughout the year. The curve consists of four loops of unequal magnitudes, giving, therefore, a quadruple point at the origin; but, besides this, the largest loop (which corresponds to the months September to December) overlaps the two adjacent loops, so that there are two double points, one on each axis. The figure might with great propriety be introduced into elementary treatises of Astronomy.

Among the presents to the Society were two handsomely bound manuscript volumes, in folio, containing Logarithms and other Mathematical Tables by the late Thomas Wright Hill, F.R.A.S.; and, in particular, Computations for a Table of Logarithms to 23 places, whereof 21 or 22 will be accurate for all composite numbers from 1 to 220000, originally commenced to 17 places for 15 true places, and now afterwards extended to the 23 places.

The volumes were presented by the author's son, Matthew Davenport Hill, Q.C.

ERRATA.

P. 52, lines 3, 4, 6. The power *should be* 474 *instead of* 174, the diameter of the field of view 5′·68 *instead of* 5″·68, and the major axis of the nebula 89″ *instead of* 89′.

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